length of each which responds to radiation is a kind of measure of specific intensity. They form (speaking sensationally) an electric eye with a definite range of colour sensation. It would be easy to supply it with a pitch or paraffin lens.

There is no need to suppose the retinal bodies to be conducting: a body of high refractive index should be subject to electric vibrations, and its surface to spurious electrifications, when radiation falls upon it; and the optical density of the rods and cones is known to be high. They may, however, be electrolytic conductors; and I find that a liquid sphere—*e.g.* a flask of inky water—responds to radiation, giving a glow to a point touching its glass.

The diameters of the rods, as measured by various physiologists, are not very different from dimensions adapted to respond to actual light-vibration frequency; and if this idea substantiates itself, these bodies can be supposed to constitute a sort of Corti's organ responding to etherial instead of to aërial vibrations, and stimulating in some still unknown, but possibly mechanical, manner, the nerve-fibre and ganglion with which each appears to be associated. OLIVER J. LODGE.

University College, Liverpool, March 11.

"Peculiar Ice-forms."

MAY I add another to the long series of communications which from time to time have been addressed to you under the above heading? Most of them have described and discussed the occurrence of ice in the form of filaments. One signed by J. D. Paul (NATURE, vol. xxxi. p. 264) seems (the description is somewhat vague) to refer to a mode of ice formation which is of somewhat frequent occurrence here, and is the only reference to this mode which I can find in that portion of the literature of physics which is accessible to me.

It happens now and again in our variable climate that a loose porous soil which has been thoroughly soaked with rain is made by a sudden and a sharp frost to produce a crop of little columns of ice. I observed a striking instance lately on a piece of hard compact ground, which, not being quite smooth, had been covered with an inch or so of loose pebbly soil for levelling purposes. Before the loose soil had been rolled or trampled upon, it became saturated with water through two days of continuous rain; and while it was still saturated, a sharp frost set in at night. In the morning the ground, to the extent of 60 square yards, was found to be covered with little columns of ice, some of them about two inches in length. They were roughly circular in section; and each column had approximately the same section throughout. Their diameters ranged from one-tenth to one-third of an inch. They were not transparent, but were whitish in appearance, and carried on their summits pebbles or frozen earth. They were thus obviously not ice crystals, such as Brewster describes in the *Edinburgh Journal of Science*, vol. ix. p. 122, as occurring in similar circumstances. The columns started from the ground at various inclinations to the vertical, and in the great majority of cases they curved upwards to a greater or less extent. I had never noticed this upward curving of the ice columns before, but other persons familiar with the phenomenon assure me they have observed it.

The explanation of this mode of ice formation seems pretty obvious. The sudden frost solidifies the crust of the soil; and it may therefore sometimes happen (in the above case it clearly must happen) that water becomes imprisoned between the frozen crust and the impervious sub-soil. Further freezing enables nature to perform Major Williams's experiment for us. If the crust does not give way as a whole, it must at its weak points; and the internal pressure is relieved by the protrusion of ice columns through apertures formed at these points. These columns would naturally carry portions of the crust on their summits, and during their protrusion might be expected to have innumerable minute fissures or cracks produced in them so as to exhibit a whitish snowy appearance. At the base of any column, at points where the freezing-point has been lowered by the pressure to the actual temperature, melting is continually occurring, and the water thus formed will flow into the fissures referred to. If the axis of the column is inclined to the vertical, and if we assume that the fissures and the points at which melting occurs are pretty uniformly distributed, more water will flow into the fissures of the lower side of the column than into those of the upper side. When the water re-freezes therefore, the lower side must elongate more than the upper, and the column must consequently in general curve upwards. That in exceptional cases the upward curving may not occur is obvious.

J. G. MACGREGOR.

Dalhousie College, Halifax, N.S., March 1.

On a Certain Theory of Elastic After-Strain.

IN a recent paper (Proc. Lond. Math. Soc., April 11, 1889), Prof. Karl Pearson has discussed at some length the possible forms of the additional terms which may be introduced into the general equations of elasticity by a consideration of the mutual action of the molecules and the ether, and has examined what physical phenomena may admit of explanation in this way. In particular, certain terms which thus appear admissible are made to yield a theory of the phenomenon known as "*elastische Nachwirkung*," or "after-strain." The attempt to explain such a comparatively slow process by the intervention of the ether certainly invites scrutiny, and in fact a very slight examination serves, I think, to show that the theory in question rests on a mistake. The author, after writing down the equations which (on his view) represent the steady application of stress to a portion of matter, proceeds to integrate them in the usual way by assuming a time-factor e^{mt}, and arrives at a quadratic in m^2 whose roots are μ/μ' and $(3\lambda + 2\mu)/(3\lambda' + 2\mu')$, where λ, μ are the ordinary elastic constants of Lamé, and λ', μ' are the coefficients of the additional terms referred to. He continues:—"Now m cannot be *positive*, so long at least as we are dealing with elastic-strain. For λ' and μ' are small as compared with λ and μ, the effects we are considering being only of the second order. Hence m^2 is large, and if m were positive the strain would rapidly grow immensely large, which is contrary to experience. Thus, we must give m the negative values $-\sqrt{(\mu/\mu')}$ and $-\sqrt{\{(3\lambda + 2\mu)/(3\lambda' + 2\mu')\}}$." The positive values of m are certainly inconvenient, but they are on the same footing with the negative ones; all are solutions of the author's equations, and all are required for the purpose of satisfying arbitrary initial conditions. The proper inference is surely that the substance is unstable, so long as the constants μ/μ' and $3\lambda' + 2\mu'$ are (as the author has tacitly assumed them to be) positive. If, to avoid this disaster, we change the signs of these constants, we get circular instead of exponential functions, and all analogy to elastic after-strain of course disappears. In its place we have *vibrations* (not molecular, but "molar") whose period is intrinsic to the substance and independent of the dimensions of the portion considered. To what physical reality these may correspond I do not undertake to say. HORACE LAMB.

The Owens College, March 4.

Foreign Substances attached to Crabs.

IF, as Mr. Garstang seems to suppose, the presence of tunicates on a crab is to be regarded as a danger-signal to its enemies, then *Hyas* must belong equally to both his groups α and β. I have found simple tunicates (*A. sordida*) on two small specimens of *H. coarctatus*. In one example they almost completely hid, and several were larger than, the crab. I do not know if anyone has observed *Hyas* "dressing" itself with tunicates. I should think it was an operation of some difficulty, at least in the case of *A. sordida*, which adheres pretty tightly to stones and shells. It cannot be said to be brilliantly coloured, so that its assumption by *Hyas* might be regarded as only an adaptation for concealment, as in the case of *Algæ*—belonging, therefore, to group α. It seems to me, however, very doubtful whether a small *Hyas* would, even if it could, willingly burden itself with such a serious incubus as half a dozen tunicates. Probably their presence is in no way due to any act of the crab's.

The shore-crab, as pointed out long ago by Prof. McIntosh, frequently suffers loss of sight by the usurpation of its orbit by a growing mussel, and the Norway lobster has been found with one eye grown over by a Polyzoan. Such foreign bodies are surely rather hurtful than protective, and the same may perhaps be said of the tunicates on *Hyas*. It is also a question whether the crab likes the smell of tunicates any better than its neighbours.

I think Mr. Garstang is wrong in assuming the inedibility of tunicates. Prof. McIntosh, in "The Marine Invertebrata and Fishes of St. Andrews," speaks of *Molgula arenosa* as being found abundantly, and of *Pelonaia corrugata* as occasionally in the stomach of the cod and haddock; and Mr. W. L. Calder-

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wood has found *Pelonaia* in some numbers in the intestine of the common dab.

Amongst anemones, *A. mesembryanthemum* is certainly a favourite food of the cod, and is not uncommon on the carapace of *Cancer pagurus*. It is difficult to see in what way the anemone is there protective to the crab. Both young crabs and anemones (of this and some other species) are equally preyed on by the cod; and though the crab may perhaps be big enough (as in a recent specimen 5 inches broad) to enjoy immunity from the cod's attack, yet, by parading such a gaudy bait, it must at least run the risk of a severe shaking. It may be added that, in the last-named case, the anemone quitted the crab, when moribund, for a more desirable basis.

ERNEST W. L. HOLT.

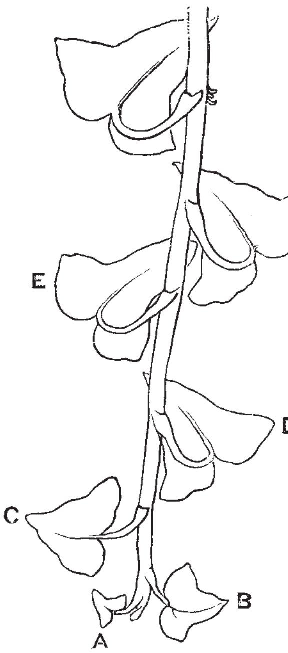
St. Andrews Marine Laboratory, N.B., March 9.

Abnormal Shoots of Ivy.

THE accompanying sketch represents a condition which is exhibited by a certain group of ivy plants in the neighbourhood of Plymouth. The plants are rooted upon the top of a high bank, which bounds the southern side of the road from Mount Edgcumbe to Tregantle; the branches pass downwards from the top of the bank on to its northern side.

The young shoots of each plant are conspicuous, because their leaves appear red, and so contrast strongly with the green of the older leaves. This appearance is due to the fact that the lower surface of each leaf is uppermost.

The sketch represents the terminal portion of a young shoot. The growing point is directed downwards. The three terminal leaves, A, B, C, have their upper surfaces directed upwards. The leaves beyond these, however (D, E, &c.), are twisted in a twofold way. First, each leaf-stalk is twisted on its own axis, so that the lower side of the leaf is directed upwards; and secondly, the apex of each leaf is rotated through 180°, so that it points away from the growing point of the shoot which bears it, towards the root.



This twisted condition is exhibited by about twelve or fourteen leaves on every young shoot—say, through a dozen inches from the growing point. The older leaves lose both kinds of torsion, so that each old leaf has its upper side uppermost, and its apex is directed towards the growing point of the stem. The under sides of the older leaves have completely lost their red colour.

The condition described is exhibited by all the shoots of a plexus of ivy plants just beyond the fifth milestone from Mount Edgcumbe, on the road above mentioned. It is absent in all the many bushes and creeping masses of ivy which grow on the same bank of the road between this point and Mount Edgcumbe. Whether all the plants composing the abnormal plexus are the offspring of a single parent cannot now be determined.

Plymouth, March 10. W. F. R. WELDON.

Earth-Currents and the Occurrence of Gold.

GOLD has been so large a factor in the prosperity and greatness of Australia, that the interesting subject of the origin of gold drifts and reefs must always possess to us something more than a purely scientific attraction. In the earlier days of the gold-fields there was among the diggers much speculation, of a scientific and semi-scientific nature, as to the processes by which Nature had produced the accumulations of coarse and fine gold dust which it was their business to extract from the alluvial drifts. The most obvious explanation, of course, was that the grains of gold had an origin similar to that of the *débris* and detritus of various characters which made up the alluvium itself; and this explanation seemed to harmonize so completely with the general processes of Nature that at one time it was almost universally accepted as the correct one. But many thoughtful mining authorities had their doubts upon the subject, and these doubts were not founded, as so frequently happens, upon mere prejudice, but were fortified by the fact that certain phenomena characteristic of the occurrence of drift gold were not only not explained by the "detrital hypothesis," as it is called, but were absolutely inconsistent with it. Chief among these objections may be mentioned the undoubted generalization that drift gold is nearly always purer than the gold in the reefs of the neighbourhood in which it occurs. No explanation as to the long distances to which grains of gold might be conveyed, or to the possible purifying effects of natural chemical action, made up any satisfactory explanation of the known facts, and accordingly under the detrital theory these facts had to remain shrouded in mystery. Then, again, it was a frequent occurrence for gold to be found so peculiarly embedded in pieces of wood, or in conjunction with natural crystals of minerals, such as the sulphides, that those who were constantly being brought into contact with such phenomena were firmly convinced that at all events there was a certain proportion of the gold found in alluvial drifts which had its origin in some other source than the breaking down of quartz reefs by the ordinary processes of Nature. The majority of those who held to this belief had at first but little scientific knowledge of natural reactions; and when questioned as to their theory on the subject, they were accustomed to say of the alluvial drift-gold, that it appeared to be actually growing—a statement which sometimes provoked, not unnaturally, a smile of pity for misplaced credulity.

These objectors, however, were right. Of this there is now scarcely the shadow of a doubt. It would be tedious to trace the steps by which such a strange conclusion has come to be virtually established. Suffice it to say that at the present day there are but few scientific men in Australia who have studied the subject who do not hold that by some agency or another the gold that is in our alluvial drifts has been formed, and probably is at present accumulating at the present moment, in its present position. It seems probable, indeed, that drift gold has its origin in the salts held in solution by the water by which it was formerly supposed to have been merely carried from one place to another. The most common salt of the precious metal is chloride of gold; and of this salt there is an appreciable quantity present in sea water along with the common sea salt, which, of course, is mainly chloride of sodium. In geological epochs, when the rocks of our present gold-fields were submerged below the ocean, and later on, when they held upon their surfaces vast imprisoned lakes of salt water, it is probable that they became saturated with sea water and retained large amounts of gold in solution. According to a computation quoted by Mr. Skey, the Government Geological Analyst for New Zealand, it is probable that every cubic mile of rock contains something like a million ounces of gold. Hence the underground streams of Australia, in certain districts, are particularly rich in salts of the precious metal, and there is an enormous area over which slight quantities of gold can always be obtained, while surface streams which are fed by deep-seated springs accumulate gold upon alluvial flats and hollows. Some of the gold found in such streams may undoubtedly be ascribed to the destruction of quartz reefs. It stands to reason that these reefs, like other rocks, must contribute to the *débris* in the beds of rivers and streams. But most of the purer alluvial coarse gold has evidently a different origin.

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